tude. The first really effective survey was that of Messrs. Lewis and Clarke in 1804–1806. In 1815 Land Office surveys were commenced. After the admission of the State to the Union in 1820 more accurate surveys were required to fix the boundary lines, but these had to be rectified in 1850, when serious discrepancies were found. Really trustworthy surveying was only begun when the Coast and Geodetic Survey commenced a triangulated line across the state in 1871. The Mississippi and Missouri River Commissioners subsequently rectified the mapping of the rivers, and now the topographical survey of the State is being carried out by the U.S. Geological Survey, which has executed maps of one-third of the area on the scale of about two miles to the inch. The Missouri Geological Survey also makes a topographical map of selected parts of the State on the scale of about one mile to the inch.

THE OBSERVATORY ON MOUNT BLANC.

As briefly announced in our Notes last week, Dr. Janssen has recently visited the observatory on Mount Blanc. In the current *Comptes Rendus* he gives an account of the expedition from a scientific point of view, and the following is a translation of his description :—

We left Chamonix on September 8, at 7 a.m., and arrived at the summit on September 11, at 2.30 p.m. The observatory was then in front of us. This construction has several floors, of which the framework, formed by large and massive beams crossed in all directions in order to ensure the rigidity of the whole, produces a deep impression upon the mind. One wonders how it has been possible to transport the edifice to this altitude and fix it on the snow. However, if the conditions offered by the hard, permanent, and little mobile snows of the summit are carefully considered, it is soon recognised that the snows are able to support very considerable weights,[1] and that they will be only slightly amenable to displacements, which will render it necessary to straighten again the construction which has been fixed upon them.

On my arrival I made a rapid survey, and saw that the construction had not been sunk in the snow as much as I had stipulated of the contractors. I do not approve of this. My guides and myself then took possession of the largest underground room. I intended at first to fix the instruments for enabling observations to be commenced immediately, and the provisions were left on the Rocher-Rouge. This circumstance put us in a state of perplexity, for the weather suddenly became very bad, and we had to remain two days separated from the stores. The storm lasted from Tuesday until Thursday morning. Beautiful weather then set in, and I was able to begin the observations.

The observations have for their principal object the question of the presence of oxygen in the solar atmosphere. The Academy knows that I worked at this important point during my ascensions to the Grands-Mulets (3050 metres) in 1888, and at M. Vallot's observatory in 1890.

But the novelty of the observations of 1893 lies in the fact that they have been effected on the very summit of Mount Blanc, and that the instrument employed is infinitely superior to that of the two preceding ascensions. At the first, in fact, a Duboscq spectroscope incapable of separating the B group into distinct lines was employed, while the instrument about to be employed at the summit of Mount Blanc is a grating spectroscope (the dispersive piece of which I owe to the kindness of Rowland), with telescopes having a focal length of 0·75 and showing all the details of the B group. This circumstance is of considerable importance, for it may lead to the discovery, in the constitution of the group in question, of valuable elements for measuring in some way the effects of the diminution of the action of our atmosphere as one ascends into it, and, accordingly, to determine whether this diminution corresponds to total extinction at its limits. In fact we shall learn whether or no the double lines which make up the B group diminish in intensity as their refrangibilities diminish ; that is, as their wave-lengths increase.

This circumstance may perhaps be employed with profit, if not to measure, at least to observe the diminution of the action of the selective absorption of our atmosphere. It has been ascertained that the most feeble doubles fade away one after the other as the atmosphere is ascended, that is to say, as the absorbing action is diminished. Thus, under ordinary circumstances, at the surface of our seas or upon our plains, thirteen or fourteen doubles can be seen, not reckoning that which is known as the head of B.

But even at Chamonix, that is at an altitude of 1050 metres, the thirteenth double is very difficult to make out, and at the Grands Mulets (3050 m.), it is only possible to see from the tenth to the twelfth, while at the summit of Mount Blanc I could hardly go beyond the eighth.

It is not to be supposed that we establish a proportionality between the numerical diminution of the doubles and that of the atmospheric action. The law is evidently of a much more complex character. But this diminution, especially when considered in connection with the experiments made with tubes full of oxygen, and able to reproduce the series of atmospheric phenomena to which we have referred, is sufficient for us to conclude that the B group would totally disappear at the limits of our atmosphere. It is remarkable, however, that if we take the co-efficient 0·566 that represents the diminution of atmospheric action at the summit of Mount Blanc according to barometric pressures $\left(\frac{0·43}{0·76} = 0·566\right)$ and multiply it by thirteen—the number that represents the doubles clearly visible on the plain—we obtain 7·4 as the result ; that is to say, very nearly the number (8) doubles that can be seen by me on the summit of Mount Blanc.

This result is certainly remarkable, but I repeat that, in my opinion, it is only by the comparison with tubes reproducing the same optical conditions as nearly as possible, that any definite conclusions will be obtained. These comparative experiments have already been commenced in the laboratory of Meudon Observatory, and they lead to the same result, viz., the disappearance of the groups A, B, and α at the limits of the atmosphere. On account of the importance of the question, however, the experiments will be repeated and completed.

The question arises as to whether the high temperatures to which solar gases and vapours are subjected are not capable of modifying the power of selective absorption, and particularly whether the absorption of oxygen which takes place in the sun's atmosphere would not be altogether different from that indicated by the experiments which have been made at ordinary temperatures.

I have already instituted experiments with the idea of replying to this objection. I shall give an account of them to the Academy in due course, but I may say that the absorption spectrum of oxygen, either the line spectrum or the unresolvable bands, do not appear to be modified in an appreciable manner when the oxygen is raised to temperatures of about 400 or 500 degrees.

On the whole, I think that observations made on the summit of Mount Blanc give a new and much sounder foundation to the study of the question of the purely telluric origin of the oxygen groups in the solar spectrum, and lead to the conclusions previously stated.

Independently of these observations I have also given some attention to the transparency of the atmosphere of this almost unique station, and to the atmospheric phenomena which are included in such an extensive view, and across such a great thickness. I shall speak of this on a future occasion.

The observatory, of course, is not completed. There yet remains much to be done independently of interior arrangements and the installation of the instruments ; but the great difficulty has been overcome, for we are free to work, and no longer have to reckon with the snowstorms ; the rest will follow in due course.

I hope that the observatory will soon be able to offer a much more comfortable sojourn than I have had there ; but that will depend upon the weather. Be this as it may, I regret nothing. I strongly wished to see our work in position, and still more fervently desired to inaugurate it by observations which are ever in my mind. I am fortunate at having been able to realise my desires in spite of some difficulties.

IRON AND STEEL INSTITUTE.

A very successful meeting of the Iron and Steel Institute has just been held in Darlington, commencing on Tuesday, September 26. The President, Mr. E. Windsor Richards, occupied the chair There was a very good list of papers, eleven in all, as follows:—On the Manufacture of Basic Steel at Witkowitz, by Paul Kupelwieser; on the Waste of Fuel, Past, Present, and Future, in Smelting Ores of Iron, by Sir Lowthian Bell, F.R.S.; on Iron and Steel at the Chicago World's Fair, by H. Bauerman; on Iron and Steel Wire, and the Development of its Manufacture, by J. P. Bedson; on the Sampling of Iron Ore, by T. Clarkson; on the Tudhoe Works of the Weardale Iron and Coal Company, by H. W. Hollis; on the Lührig Coal Washing and Dry Separation Plant at the North Bitchburn Coal Company's Randolph Pit, by James I'Anson; on Carbon in Iron, by Prof. Ledebur (Freiberg); on Suggested Improvements in connection with the Manufacture of Steel Plates, by William Muirhead; on the Last Twenty Years in the Cleveland Mining District, by A. L. Steavenson; on the Production of Wrought Iron in Small Blast Furnaces in India, by T. Turner.

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[1] See *Comptes Rendus* for an account of experiments made at Meudon on the resistance of slightly compressed snow.

Mr. Kupelwieser's paper was first taken, and gave an interesting account of the basic process, which the author has introduced with considerable success at Witkowitz. It is of course well known that the cheapest process of steel manufacture is that carried out with the Bessemer converter; the Siemens furnace comes next. Although the basic process was originally devised for the use of the Bessemer converter, it has had considerable application with the open-hearth furnaces. The basic process is, however, dearer than the old acid system of manufacture, and some of the most beautiful mild steel produced is made in the open-hearth furnace with a basic lining. The latter, again, is more costly than the old acid lining, and we have therefore the following gradations as to cost:—First, the acid Bessemer, or original process; next, the basic Bessemer; then the acid open-hearth, and finally the basic open-hearth; the latter being the dearest of the four. It is obvious therefore, that when circumstances permit it, that the Bessemer converter should be used in place of the open-hearth, but there is an objection to the Bessemer converter in the fact that it acts so much more quickly; the slower working open-hearth furnace giving time for tests to be made, and the product is consequently more certain. Mr. Kupelwieser has introduced a combined process in Witkowitz, which many members of the Iron and Steel Institute saw in work, when the Institute meeting was held in Austria. The pig-iron obtained in Witkowitz contains too much phosphorus for use in the ordinary Bessemer process, while it does not contain sufficient phosphorus for the basic process. As a further complication, a supply of cheap scrap was not obtainable at the works. The way in which the author got over his difficulties is highly ingenious, and is well worth study by English steel-makers. The pig-iron is run from the blast furnace into the ladle, and transferred immediately to the converter, which has an acid lining. The blast is then turned on, and the blow kept up till the pig-iron is de-siliconised; an operation that requires about five or six minutes to perform. The silicon being removed, manganese and a considerable amount of carbon remain; the metal is poured into a ladle, and taken to the open-hearth furnace, where the process of steel-making is completed. This method of operation has the great advantage that the molten metal, when run into the open-hearth furnace, which is basic lined, does not destroy the lining, as it has become completely de-siliconised. The time required for working the charge is considerably diminished, and the amount of iron taken up by the slag is said to be less, as also are the expenditure of fuel and cost of wages. We do not propose following the author into his figures as to the cost of production, but the balance in favour of his method is 10*s.* per ton as compared with the cost of conversion in the open-hearth furnace from the commencement, whilst it is said to be no dearer than the basic Bessemer process when carried out on a large scale. Perhaps the chief point of interest to English steel-makers is the working of the metal from the blast furnace direct; a thing which has of course often been considered by steel-makers in all parts of the world. It is always difficult, and often fallacious, to make comparisons unless the whole conditions on both sides be similar, and it is certain that those conditions existing at Witkowitz have not their exact counterpart in this country. It is not surprising, therefore, to find that during the discussion of the paper high authorities differed. Mr. James Riley did not by any means approve of the author's suggestions; basing his objection principally on the waste that would take place in the process. On the other hand, Mr. Snelus and Mr. Whitwell—both high authorities—supported the author. There is of course the undeniable 10*s.* a ton, which is sufficient vindication of the process as carried out in Witkowitz. Whether the 10*s.* would still be to the good in English steel works, is a matter that is open to question.

Sir Lowthian Bell next read a paper on the waste of heat, past, present, and future, in smelting iron ores. This contribution was largely of a historical nature. Its scope is sufficiently indicated by the title, and it would be a useful intellectual exercise for students to follow its reasoning.

Mr. Bauerman's paper on the iron and stsel exhibits at the Chicago Exhibition was of the usual nature of such papers. The principal wonders to be seen were referred to by the author, but it is unnecessary for us to follow him in his description. The same remark applies to Mr. Steavenson's paper, with the reading of which the first day's sitting was brought to a close. In the afternoon a very instructive and pleasant excursion was made to the Weardale Steel and Iron Company's works at Spennymore, where members had an opportunity of seeing the gigantic operation of cogging and rolling ingots, which are characteristic of the modern steel works.

The first paper taken on the second day's sitting was Prof. Ledebur's contribution on carbon in iron. This met with a mixed reception, the opinion of some members appearing to be that it was hardly worthy of the time allotted to it, although international courtesy forbade them blankly saying so. We think such an opinion can only be due to a cursory study of the paper, which appears to be one of considerable value, and especially suitable for the Transactions of the Institute. There is some controversial matter in this memoir, but its value is that it brings together in a very compact form many of the leading facts involved in the subject upon which it treats, and although there is not much in it that is new—in fact, the matter consists of that already known—yet many of the details are, as was stated by Mr. Hadfield, the result of the author's own research. The paper is well worth the consideration of metallurgists, and we think there are few who would not benefit by their memories being refreshed, even if the facts were not altogether new. Prof. Ledebur holds that there are four states of carbon in iron. The first is the graphitic state; the second, that which the author described as the temper-carbon; the third is a carbide carbon or cement carbon; and the fourth hardening carbon. It will be evident that in stating this the author opened up matter largely of a controversial nature, and in the discussion which followed, Mr. Snelus, Mr. Hadfield, Prof. Roberts-Austen, Mr. Stead, Sir Lowthian Bell, Prof. Thomas Turner, and Mr. Edward Riley took part. It is unnecessary to say that with all these gentlemen engaged in the discussion there was some clashing of opinion. Mr. Hadfield stated a most interesting fact, in that from a malleable iron casting he had obtained three per cent. of graphitic carbon; but we believe the casting showed all the physical qualities of having been well annealed. The result is of course not difficult to conceive, but the fact is none the less of interest. The action of silicon in regard to carbon in steel also occupied the attention of the meeting during this discussion, but we did not notice that any new facts were brought forward.

Mr. Bedson's paper on iron and steel wire was a very able contribution on a most interesting subject. The author is of the fourth generation of wire-makers, the business in which he is engaged having been handed down to him from his great-grandfather; whilst his father added some of the most important improvements to the machinery and process of wire manufacturing. The immense superiority of basic steel over that produced by the acid process was strongly insisted upon by the author; a fact which called forth some rather sharp remarks from Mr. James Riley, who protested against the acid Siemens steel being left unmentioned, as by far the greatest quantity of steel wire was manufactured from that metal. Mr. Bedson's paper was a long one; but if it had been even more extended, his audience would not have objected to it. For our own part, we should have been glad to have seen some mention made of the extraordinary tenacity in steel produced by drawing into wire.

The next paper taken was that of Mr. William Muirhead. Unfortunately this contribution was written in such a way that the author's meaning was somewhat obscure. From the wording of the paper we gathered that Mr. Muirhead would abolish the cogging process by which the ingot is broken down in rolls to manageable dimensions for rolling. This operation was originally performed by the steam hammer, and the cogging rolls were undoubtedly a great improvement, enabling work to be done with more expedition and greater cheapness. The author in his paper certainly advocated abandoning both hammering and cogging. In his paper he said, "Cogging, as it is at present carried on, with its consequent reheating, is a cumbersome, almost an ugly operation, and from the arguments I have endeavoured to adduce, an unnecessary one. How much smarter and cheaper it will be to take the ingots and roll them right off into plates, and I commend this to your earnest attention." Yet in the discussion which followed, Mr. Muirhead said that he did not in his system do without cogging. The point is one of considerable importance, and, Mr. Muirhead's position as the manager of an important steel-producing plant commands for him attention. If the same results can be got from the ingot without cogging and reheating, undoubtedly a great step in advance will have been taken; but the majority of steel-makers—perhaps we might say all, with the exception of Mr. Muirhead—think that cogging or hammering is a necessary though expensive process. Of course, if the author can show that he is right, and the rest of the steel world wrong, he will have performed a signal service to the industry. If we were the owners of steel works, however, we should prefer the experiments to be carried out by other manufacturers. It may be added that what is known as the direct process of rolling is not a new thing, and for Mr. Muirhead to succeed he will have to introduce some entirely fresh element into his procedure.

The last paper read at the meeting was Mr. Clarkson's contribution, in which he described his ore sampling machine. It would seem a small matter, at first glance, to sample ore, but it is by no means an easy thing to do. The variations in quality or composition are arbitrarily distributed, and it may easily be that a sample made up from portions from several different positions in the mass to be sampled, may not be a fair representation of the whole. Machines have been before used, by means of which small portions of a falling mass of ore may be abstracted at regular intervals. It would be difficult to describe this device without the aid of diagrams, but it may be stated that though they appear to work fairly and equitably at first sight, they are in reality partial in their selection. Mr. Clarkson has brought a trained mind to bear upon this subject, and has produced a really scientific instrument. The mass of ore is caused to fall in an annular stream, descending into a hopper, which is made to revolve at great speed. By a suitable mechanism small portions of the ore are abstracted at regular intervals, and from the fact that the falling mass takes the form of an annulus in place of a solid stream, the tendency of certain qualities to gather in the middle of the stream is obviated. A small-sized apparatus was shown in the theatre, and the author was able to practically demonstrate the accuracy with which it worked, so far as the exact percentage of the material abstracted from the whole was concerned. The demonstration, it may be said, was perfectly successful. The apparatus has another useful field in distribution of a mass into equal parts, so that by it a number of bottles or boxes can be filled without the tedious process of weighing being gone through, and yet each receptacle will have its due share of the material. The error of the ore separator is less than at present.

This was the last paper read at the meeting, which concluded with the usual votes of thanks.

THEORIES OF THE ORIGIN OF MOUNTAIN RANGES.

In his presidential address, delivered before the American Association for the Advancement of Science this year, Prof. Le Conte dealt with theories of mountain genesis—a subject which lies at the very foundation of theoretical geology. Want of space forbids us printing the address in full, but the most salient points are contained in the extracts from it that are here given.

Prof. Le Conte began by stating those fundamental features of the structure of mountain ranges on which every true theory of their origin must be founded. These features are: (1) Thickness of mountain sediments; (2) coarseness of mountain sediments; (3) folded structure of mountains; (4) cleavage structure; (5) granite or metamorphic axis; (6) asymmetric form. Another type of mountain, the main characteristics of which are not included under the above heads, are those only found in the Basin and Plateau regions, and therefore termed the Basin region type. In fact, "mountains may be divided into two types, viz. mountains formed by folding of strata, and mountains formed by tilting of crust-blocks. The structure of the one is anticlinal or *di*clinal, of the other, *mono*clinal. The Sierra probably belongs to both types. It was formed at the end of the Jurassic as a mountain of the first type, but the whole Sierra block was tilted up on its eastern side without folding at the end of the Tertiary, and it then became also a mountain of the second type. A complete theory must explain this type also; but since from the exceptional character it must be regarded as of subordinate importance, we shall be compelled to confine our discussion to mountains of the usual type."

Before going any further, however, Prof. Le Conte made a digression in order to clearly lay down what he meant by theory. After facts have been collected they must be explained, and the explanation, which merely gives the laws of the immediate phenomena in hand, is called the *Formal Theory*. The next step towards the perfection of knowledge consists in explaining the cause of these laws, and is termed the *Casual* or *Physical Theory*. The following is an illustration of this distinction:—

"All the phenomena of the drift are well explained by the former existence of an ice-sheet moving southward by laws of glacial motion, scoring, polishing, and depositing in its course. This is the formal theory. But still the question remains, What was the cause of the ice-sheet? Was it due to northern elevation, or to Aphelian winter concurring with great eccentricity of the earth's orbit? And if due to northern elevation, what was the cause of that elevation? A perfect theory must answer all these questions."

". . . . I wish to keep clear in the mind these two stages of theorising in the case of mountain origin. The formal theory is already well advanced toward a satisfactory condition; the physical theory is still in a very chaotic state. But these two kinds of theories have been often confounded with one another in the popular and even in the scientific mind, and the chaotic state of the latter has been carried over and credited to the former also; so that many seem to think that the whole subject of mountain origin is yet wholly in the air, and without any solid foundation."

Bearing in mind that "a true formal theory, keeping close to the immediate facts in hand, must pass gradually from necessary inferences from smaller groups to a wider theory which shall explain them all," Prof. Le Conte showed the inferences that could be made from the characteristic features of mountain structure, and he then grouped those inferences, and summed up his views as to the mode of mountain formation as follows:—

Summary Statement of the Formal Theory.

(1) "Mountain ranges, while in preparation for future birth, were marginal sea-bottoms receiving abundant sediment from an adjacent land-mass and slowly subsiding under the increasing weight. (2) They were at first formed and continued for a time to grow, by lateral pressure crushing and folding the strata together horizontally and swelling them up vertically along a certain line of easiest yielding. (3) That this line of easiest yielding is determined by the hydrothermal softening of the earth's crust along the line of thickest sedimentation. (4) That this line by softening becomes also the line of greatest metamorphism, and by yielding the line of greatest folding and greatest elevation. But (5) when the softening is very great, sometimes the harder lateral strata are jammed in under the crest, giving rise to fan-structure, in which case the most complex foldings may be near but not at the crest. Finally (6) the mountains thus formed will be asymmetric because the sedimentary cylinder-lenses from which they originated were asymmetric."

Several American examples illustrating these views were then given, and it was shown that eruptive phenomena, faults, mineral veins, earthquakes, and other minor phenomena associated with mountains are well explained by them. To quote Prof. Le Conte: "Leaving out the monoclinal type, which seems to belong to a different category, all the phenomena, major and minor, of structure and of occurrences, connected with mountains, are well explained by the theory of *lateral pressure* acting on lines of thick sediments accumulated on marginal